DISCUSSION BEFORE THE WIRELESS SECTION, 7TH DECEMBER, 1932.

Dr. R. L. Smith-Rose: I am particularly interested in the subject of the paper because several years ago Mr. Barfield and I* attempted to obtain some measurements of the resultant magnetic and electric forces of a wave arriving at a receiving station, by measuring certain quantities concerned with the magnitude and direction of those forces. In those days we had to work with much simpler apparatus, which was more complicated to operate than that used by the author. We used one set of apparatus to measure the direction of the magnetic field, and two more sets to measure the direction and intensity of the electric field respectively. These three sets had to be operated by different observers, and it was with some difficulty that we were able to synchronize the observations so that the measurements of all the quantities were carried out at the same time. As we were working on rather longer wavelengths than those dealt with in the paper, the phenomena did not vary quite so rapidly, and we were able to get some useful results from our measurements. The author has had the advantage of that useful tool in radio research, the cathode-ray oscillograph, which enables one to see on a screen what is happening in space to the electric field which comprises the arriving wave. One of the most interesting results that has emerged from the paper is the unexpectedly small angle of incidence of the waves received from Moscow. The author gives an interesting explanation of the way in which this could occur without unduly disturbing our present conceptions of the manner in which the reflecting layers operate.

Prof. L. S. Palmer: The author's great difficulty is to explain the small eccentricity of some of his ellipses without having to assume that the angle of incidence of the downcoming ray is smaller than the simple reflection theory would lead one to expect. Therefore, before accepting the conclusion to which he is forced, I should like to ascertain whether two other possible explanations have been effectively disposed of. (1) To what extent can the reflected ray from the ground produce the observed ellipses? If the coils are influenced by such a reflected ray as well as by a direct downcoming ray, then the observed effects can be produced without invoking the refractive properties of the ionosphere. It is not safe to assume, as is done in the paper, that a downcoming circularly-polarized ray is distorted after reflection from the imperfectly conducting ground, and alone affects the receiving aerial. In the course of some recent experiments at Hull it was found that this reflected ray was not only instrumental in producing elliptically polarized fields but could be artificially varied by thoroughly soaking the ground under the aerial with water. I think, therefore, that such considerations as these might materially affect the eccentricity of the ellipse observed in the present experiments. (2) The second factor which may affect the present results is the nature of the changing polarization of the incident ray when in the Heaviside layer. It is assumed in the paper that the incident ray from the transmitter is polarized perpendicularly to the plane of incidence. By the time the incident ray has emerged from the ionosphere, however, its plane of polarization will have been changed. Any rotation of the plane of polarization will, for a given angle of incidence, tend to reduce the phase difference between the components of the emergent rays and so tend to broaden the ellipse. Thus, depending on the extent of the rotation, the calculated angle of incidence to produce a given phase difference may be considerably larger than the author's calculations would lead one to expect. This second question should, I think, be settled before we accept the conclusion that a very small angle of incidence is the only solution which will account for the small eccentricity of some of the author's ellipses. In conclusion, I think the ultimate interpretation of these important experiments will appreciably add to our knowledge of the mode of propagation of wireless waves in general, and will still further confirm the magneto-ionic theory of propagation in the ionosphere in particular.

Mr. J. A. Ratcliffe: The author's measurements give us good reason for believing that short waves from a distant transmitter are incident at the receiver more steeply than has usually been supposed. It is interesting to speculate on the reason for this; we must not, I think, exclude the possibility of an asymmetrical ray leaving the transmitter at a very large angle with the vertical (i.e. nearly horizontally) but incident steeply at the receiver after only one deviation in the ionosphere. It may be that the experiments of Walmsley,* who finds that for these short waves the rays leave the transmitter at an angle of 10° to the horizontal, support this idea of an asymmetrical ray. For the convenience of those who may wish to test theories of wave propagation by applying the present author's results, I suggest that he should add to his paper a map giving the location of the various transmitters used. I cannot agree with the fundamental reasoning put forward by the author in connection with the last method described for "ray suppression," in which the signal is received independently on a loop and on a vertical aerial, and, after rectification, the two unidirectional currents are sent in opposite directions through a galvanometer. To make the point clear, let us suppose that a signal of amplitude E_1 is arriving at an angle θ_1 with the vertical; then the e.m.f. across the rectifier connected to the aerial will be proportional to $E_1 \sin \theta_1$, while that produced across the loop rectifier will be proportional to E_1. If we suppose that both rectifiers obey a square law, the unidirectional components of the currents from the rectifiers are given by $\alpha(E_1^2/2) \sin^2 \theta_1$ and $\beta(E_1^2/2)$ respectively, where α and β are circuit constants. If these rectified currents are fed together through a d.c. instrument, and if we arrange the circuits, so that

$$\alpha \sin^2 \theta_1 = -\beta$$

then there is no resultant current whatever the value of E_1, i.e. we have suppressed the ray arriving at an angle θ_1 with the vertical. Although I am so far in

* R. L. SMITH-ROSE and R. H. BARFIELD: *Proceedings of the Royal Society*, A, 1926, vol. 110, p. 580.

* *Nature*, 1932, vol. 130, p. 814.

complete agreement with the author, I do not agree that the same circuit conditions will necessarily lead to suppression of the ray (E_1, θ_1) in the presence of a second ray (E_2, θ_2) making a different angle with the vertical. Thus if ϕ is the phase difference between E_1 and E_2, the unidirectional components of the currents from the rectifiers are

$$\alpha(\tfrac{1}{2}E_1^2 \sin^2\theta_1 + \tfrac{1}{2}E_2^2 \sin^2\theta_2 + E_1E_2 \sin\theta_1 \sin\theta_2 \cos\phi)$$

and

$$\beta(\tfrac{1}{2}E_1^2 + \tfrac{1}{2}E_2^2 + E_1E_2 \cos\phi),$$

and if the circuits are adjusted so as to suppress (E_1, θ_1) when this ray exists alone, so that

$$\alpha \sin^2\theta_1 = -\beta;$$

then in the presence of the second wave (E_2, θ_2) the resultant galvanometer current will be

$$\beta\left[\tfrac{1}{2}E_2^2\left(1 - \frac{\sin^2\theta_2}{\sin^2\theta_1}\right) + E_1E_2\left(1 - \frac{\sin\theta_2}{\sin\theta_1}\right)\cos\phi\right]$$

If now the wave E_2 remains constant but the amplitude E_1, or the phase ϕ, of the first wave varies, then the resultant current will vary, i.e. we have not suppressed the effect of the wave (E_1, θ_1). This argument is in no way altered if a "straight line" rectifier is used, as may easily be seen by considering the intensity of the signal obtained by vectorial addition of the two incident waves. In fact, all rectifiers, including the straight-line rectifier, act in virtue of a non-linear relation between the input e.m.f. and the output current, and this inevitably involves "cross-product" terms of the kind indicated for the square-law detector. In the calculations of Appendix I the author assumes that the two waves incident on the receiver have polarizations which are characterized by two similar ellipses having axes in, and at right angles to, the vertical plane, and that the two ellipses are similarly situated, i.e. both have their major axes in the same direction. It seems to me that these assumptions are not in accord with the magneto-ionic theory, which tells us that the ellipses characterizing the polarizations of the two waves have axes in, and at right angles to, the plane containing the incident wave normal and the earth's magnetic field, and further that their major axes are mutually perpendicular, as represented in Fig. A. The author's assumptions therefore seem to be incorrect both with respect to the position of the axes of the ellipses and with respect to their mutual orientation. It would be interesting to find out whether his observations could still be explained by assuming the states of polarization which I have here outlined. In Appendix II he deals with two waves incident simultaneously at different angles and received on a loop aerial and a vertical aerial. He claims to have shown that if only two waves are present the signal maxima must occur simultaneously on both receivers, and that the same is also true for the signal minima. It seems to me that this argument is unsound, because only phase variations are considered. If it is supposed that variations in the relative amplitude (E_1/E_2) may take place without the phase changing appreciably, it is easy to show that the signal variations need not be "in step" on the two receivers. The footnote on page 248 does not deal with the point here at issue. It is concerned with the possibility of signal variations, due chiefly to phase-changes, being interfered with by amplitude changes occurring as a subsidiary effect. The variations which I envisage here are due primarily to amplitude changes, the phase remaining constant or only varying a small amount. Experience with "pulse" transmissions leads us to suppose that rapid variations of amplitude do actually take place, so that effects of this kind must occur, and I therefore do not consider that records like Fig. 10 necessarily indicate the presence of more than two rays. In the past, some confusion has arisen from the fact that when describing the polarization of a wave some authors imagine it to be viewed along the direction of travel, while others imagine it to be viewed in the opposite direction. I should be glad if the author would confirm that the direction of viewing adopted in the paper is

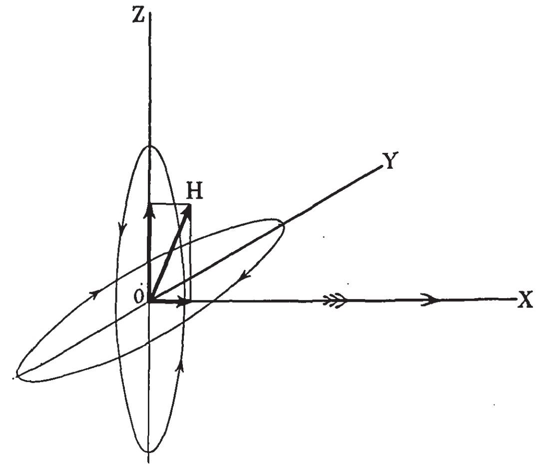

FIG. A.
OH represents earth's magnetic field in plane XOZ.
Plane ZOY is wave-front.
OX is direction of travel of wave.

that used in Prof. Appleton's original work* on this subject, i.e. along the direction of propagation.

Dr. E. H. Rayner: I should like to ask whether it would be possible to obtain further useful information by changing the frequency of the distant transmitting station. Would the angle at which one obtains grazing incidence at the lower layer be changed appreciably by small changes in the emitted frequency?

Mr. S. B. Smith: An experiment recently carried out by Mr. T. L. Eckersley† helps one to understand the mechanism responsible for the splitting of a single pulse during transmission through a magneto-ionic medium. A pulse entering such a medium can be resolved into two oppositely polarized components; the velocities of the two components through the medium need not be the same, and consequently a pair or even four pulses may be recorded upon arrival at the receiver. By adopting suitable phasing arrangements in a radio-goniometer Mr. Eckersley was able to eliminate either pulse. The persistence of certain ray angles (sometimes angles of incidence of 30°) has been noted in many

* *Proceedings of the Royal Society*, A, 1928, vol. 117, p. 576.
† *Nature*, 1932, vol. 130, p. 398.

transatlantic facsimile records, and in this connection one is often amazed at the apparent stability of the medium from the echo point of view although fading may be excessive. I should like to ask whether the author has made a detailed study of scattered signals. Some of the results obtained on such signals might be difficult to analyse by the methods described in the paper.

Prof. J. Hollingworth (*in reply*): In reply to Dr. Smith-Rose I should like to put on record that it was his work which first directed my attention to the problem of polarization.

In reply to Prof. Palmer, both the incident and reflected rays are taken into account. In the formula on the middle of page 235 the term $1 + \rho_v$ occurs, and in this the 1 is the coefficient of the incident ray and ρ_v that of the reflected ray. The object of these calculations and of the table at the top of page 236 was to show that while the effect of the earth's conductivity on the reflected wave was to broaden the ellipse formed by the combination of the direct and reflected waves, this effect was not sufficient to account for the degree of broadening actually observed without assuming figures for κ and σ enormously different from those usually accepted. Moreover, while this effect can certainly produce an ellipse on the tube it would be difficult to make it responsible for the regular and rapid cyclic variations. With regard to the second point, no assumption has been made as to the polarization of the incident ray. The form of polarization for free propagation varies throughout the path, and all that can be said is that the component magneto-ionic waves are circularly polarized after their emergence. It is, of course, the variations inside the layer which cause the phase-changes, and it is always possible to obtain from these small momentary ellipses of any broadness, but assuming both components of the magneto-ionic ray to be circularly polarized the ratio of the maximum deflections on the tube in the two directions (not necessarily simultaneously) cannot exceed a figure determined by the angle of incidence.

In regard to the positions of the ellipses of polarization it is true, as Mr. Ratcliffe observes, that their axes have special directions. On emergence from the ionized layer, however, the component of electric force along the direction of propagation is negligible (Goldstein, *loc. cit.*, p. 273), consequently the plane of both ellipses must lie in the wave-front. I am obliged to him, however, for pointing out that the major axes are at right angles. In the particular case under consideration, where the polarization is circular, this of course does not matter so that the deductions made are still true, and it merely causes a permanent shift of 90° in all the relative phases. As, however, it causes the generalization originally given in Appendix I, while still remaining sound in itself, to be inapplicable to magneto-ionic propagation, I have modified it to deal exclusively with circular polarization.

Appendix II definitely deals with phase-fades, the justification for this being that all the experiments tend to show that the rapid periodic variations such as it is applied to are phase-fades, and that intensity fades are much slower and more irregular. Figs. 8(*b*) and 8(*c*) show very clearly the distinction between these two types.

It should be noted that in work of this nature a considerable amount of discretion has to be used in the interpretation of results. The direction of rotation when applied to polarization is taken in the same direction as Prof. Appleton takes it, i.e. when looking in the direction of travel.

Mr. Ratcliffe's comments on the accuracy of the ray-suppression system are sound, and in the paper I have made certain modifications. From an experimental point of view I should, however, like to emphasize the fact, referred to on page 241, that a theoretically sound method of obtaining a " sweep " of angle of suppression is impracticable instrumentally. Consequently one is faced with the alternatives of either abandoning the attempt altogether, or using a system which, while not rigidly accurate, can give results of some value if used with discretion.

Very little work has yet, as far as I know, been done on the analysis of the angles of incidence of down-coming rays, so that even approximate results may be of considerable value.

In regard to Dr. Rayner's inquiry, the wavelength-change method comes more into Prof. Appleton's province than mine. At long distances, however, there is always the question as to whether the path difference is sufficient for its use owing to the large angle of incidence on the layer, though this could only be settled by experiment. On the practical side there is some difficulty in organizing experiments of this type from a transmitter a long way off.

In reply to Mr. Smith, I have not yet made any study of scattered signals, though such a thing would be of great interest. I have rather for the present tried to avoid them in order to reduce the problem under immediate consideration to the simplest possible terms.